The Journal of HEREDITY

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Boy or Girl, Which ... Achieved by T...

By Mr...

Acidic Girls, Bicarbonate Boys. Through Dr. Sanders (see above) *Dr. F. Unterberger* of Koenigsberg sent word that if women drink great quantities of bicarbonate of soda, they will bear only sons. A lactic acid diet produces only girls. Dr. Unterberger verified his theory with 78 daughter...

PRESS VIEWS ON "SEX-FIXING NEWS"

(See Page 265)

VOLUME 24 JULY, 1933 NUMBER 7

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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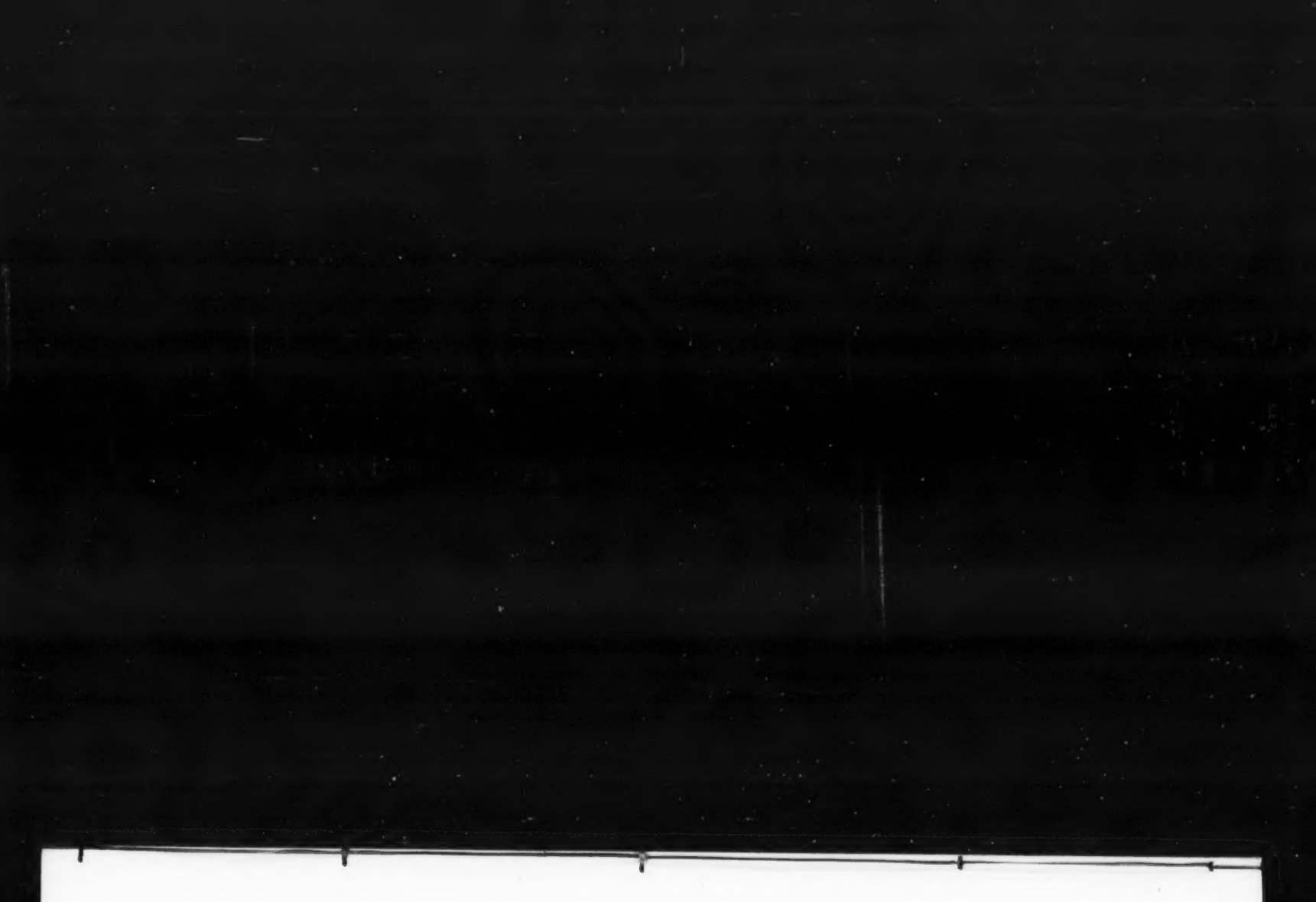

The

Journal of Heredity

Vol. XXIV JULY, 1933 No. 7

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COMING

John Belling
1867-1933

An appreciation of a worker with cells and chromosomes who made fundamental contributions both to genetical theory and cytological technique.

Van Recklinghausen's Disease

A form of "elephantiasis," undoubtedly due to an hereditary tendency to abnormal growth of the peripheral nerves and the nerve sheaths.

"False Seedlings" in the Blackberry

Psedogamy in certain species results in plants all exactly resembling the female parent.

Endocrine Treatment as a "Gene Tester"

The breeder's problem is to separate the "carriers" which look exactly like true breeding individuals but which differ in their progeny. Endocrine injections bring to light certain recessive genes in the fowl, which fact greatly simplifies the breeder's problem.

Wheat-Couch Grass Hybrids

Hybrids between different genera are always matters of interest, but *fertile* genus hybrids are "news" indeed.

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...ence
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...Secrets.

Boy or Girl, Whichever You Like, Achieved by Two Noted Russian Scientists

...ntrol of Sex

MEANS TO CONTROL UNBORN'S SEX SEEN

Use of Lactic Acid and Bicarbonate of Soda Reported Successful.

Acidic Girls, Bicarbonate Boys. Through Dr. Sanders (see above) *Dr. F. Unterberger* of Koenigsberg sent word that if women drink great quantities of bicarbonate of soda, they will bear only sons. A lactic acid diet produces only girls. Dr. Unterberger verified his theory with 78 daughter-surfeited German women. All but one bore sons. The ex- ...on had broken her alkaline régime. ...d lactic acid each produced a

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...sperm cells. Some of them contain what is called a sex chromosome X, others do not. The ones which have this X. The they fertilize the female ... result in the birth animals. The female young ...

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...enable a ...r a girl—

...ion of males

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...by the Associated Press. ITHACA, N. Y. ... which ... children ...

NEWS ON SEX DETERMINATION

Frontispiece

Headlines and introductory paragraphs of a few newspaper announcements on sex-determination, characteristic of the enthusiasm with which modest efforts sometimes are received by the press. The "Prenatal Announcement" of the expected arrival of Miss Chemical Dudding (synthesized from an infusion of cherry leaves!) has recently appeared in the Hearst papers. The clippings reproduced here are from the following sources: *The New York Times, The Boston Transcript, The Los Angeles Times, The Washington Evening Star, The Washington Herald, The Washington Times,* and *Time,* "the weekly news magazine."

SEX CONTROL AGAIN

By L. J. Cole and Ivar Johansson*

Department of Genetics, University of Wisconsin

AMONG perennial announcements that occur, particularly in the popular press, are the discovery of the "germ" of cancer, cures for tuberculosis, and methods for the control of sex. If cancer is caused by a germ it no doubt will sometime be discovered; it is not improbable that a specific cure for tuberculosis may some day be found; and sex of offspring will be under control just as soon as someone discovers a method of effectively separating the male- and female-producing spermatozoa, thus determining which shall fertilize the egg. But in the meantime the popular interest in these subjects is so great that the slightest hint of success is seized on by ambitious reporters and journalists and the public is led to believe that unqualified success has been attained.

Immediately following the Sixth International Genetics Congress held at Ithaca, N. Y., last August a wave of publicity concerning a purported new method of controlling sex of children swept this country and Canada. It is safe to say that not only did this topic occupy more space in the daily press, but it also got more popular attention and interest, than all other proceedings of the Congress together. The Editor of this Journal and many geneticists were swamped with inquiries asking for information as to the validity of the claims and for details of procedure, until attempts to answer them became a burden. It was even learned that in at least one case an unscrupulous advertiser offered, on the basis of the claims put forward, to give to breeders (for a consideration) advice as to how to produce heifer or bull calves at will.

The newspaper reports were allegedly based on a paper purported to have been read at the Congress by one Dr. J. Sanders of Rotterdam. No such paper was listed, however, on the printed program nor does it appear in the official Proceedings of the Congress. Incidental inquiry among geneticists who attended the Congress has so far failed to find one who heard the paper read. The chairman of the program committee reports, however, that a last-minute request of Dr. Sanders was granted and that the paper was presented at the end of one of the section meetings; but there appears to be no official record of it. According to a report of the Congress by Macklin[10] in the *Canadian Medical Association Journal*, the title was "Control of Sex."

Since very extreme and positive statements appeared in the newspapers, it seemed important to know just what claims Dr. Sanders had made and what basis he had for them. We therefore wrote to Dr. Sanders on March 24, 1933, stating that his paper had aroused wide publicity and asking for information as to just what he had presented and with what evidence. We have never received a response to our inquiry. Similarly, the Editor of this Journal, writing for information as to how to answer the letters coming to his office, was unable to obtain any statement as to what had actually been presented.

Under these circumstances, without a knowledge of just what statements were made, it would be unprofitable to discuss in detail the claims of Dr. Sanders as reported in the press. It appears, however, that his statements were based, not on any results of his own, but on publications of Dr. F. Unterberger of Königsberg, Germany. Whether he presented Dr. Unterber-

*Papers from the Department of Genetics, Wisconsin Agricultural Experiment Station, No. 158. Published with the approval of the Director of the Station.

ger's published statements correctly we are unable to determine, but it may be significant that in one of his papers Dr. Unterberger complains that there has been much misrepresentation of his claims. It seemed that it might be worth while to examine Dr. Unterberger's results carefully and to present a critical résumé of them to the readers of the JOURNAL OF HEREDITY. Dr. Unterberger's papers were not all available to us, but in response to our request he kindly sent us a set of reprints, which we assume to cover all that he has published on this subject. Their titles will be found in the bibliography at the end of this review.

Unterberger's Claims

In his first paper Unterberger[16] reported that in treating women for temporary sterility he had observed that the acidity of the vaginal secretion often was abnormally high, and in such cases had recommended washing the vagina with a weak sodium bicarbonate ($NaHCO_3$) solution or powdering the *glans penis* with $NaHCO_3$ powder just before coitus. In 53 cases of childbirth, spread over a period of 10-12 years, after such treatment the child was said in every case to be a male. In most cases during later years the treatment was prescribed by Unterberger for the definite purpose of controlling the sex of the child. In 1932 he reported that he then had 74 cases on record, and all were "an unbroken series of boys." It is true one exceptional case was reported but Unterberger does not feel sure that his advice was carefully followed in this case. From the earlier papers[16, 17] it is not quite clear *how* he thought the acidity of the vaginal secretion might influence the sex ratio, but in 1932 he declares himself standing "on the ground of the modern zoology and fully recognizing the importance of the *X*-chromosomes." As brought out clearly in another paper,[19] it is assumed that a slightly alkaline reaction stimulates the movement of the *Y*-bearing more than the *X*-bearing spermatozoa, thus giving the former a better chance of reaching the ovum. Such a result would favor the production of boys, while in an acid medium it is assumed the *X*-bearing spermatozoa would have the advantage, resulting in a preponderance of girls. Nowhere does he claim, apparently, that the reaction of the vagina, and thus indirectly sex, may be influenced by the acidity or alkalinity of the diet, but this seemed to be the widespread impression disseminated by the reports of Dr. Sanders' communication at the Genetics Congress (see Frontispiece).

That Unterberger is not too familiar with modern genetics is, however, repeatedly revealed in his discussion and seems quite apparent in his own summary: "The alkalinity of the male semen and the acidity of the vagina counterbalance each other in the majority of marriages, so that the chances for boy and girl become equal. This may also explain why one dizygotic twin may be a boy and the other one a girl. In a small percentage of marriages a strongly alkaline partner gets a wife with slightly acid vaginal secretion. Such marriages produce only, or predominantly, boys. On the other hand there is also a small percentage where a slightly alkaline man produces almost only girls when the wife has a high lactic acid content in the vagina." But the established frequency of sex combinations in human families produced by single births, as well as the corresponding frequency in dizygotic twinning, agrees very well with what is naturally to be expected from the purely chance combination of *X* and *Y* spermatozoa with *X* ova. Hence Unterberger's hypothesis is entirely unnecessary in order to explain why only girls are born in some families and only boys in others. A simple and inexpensive experiment in tossing the same number of coins as there are children in the family would soon make this point clear. If we select at random families, let us say, of five children, it is to be expected, purely on the basis of chance, that one family in every 32 families of this size will

TEN CHILDREN—ALL BOYS

Figure 1

To account for the occasional large family in which all the children are of one sex it is not necessary to make any addition to the accepted mechanism of sex-determination. Purely on a basis of chance, in families of ten children we should expect one family out of 1,024 to be all boys and one all girls. The extreme rarity of such an occurrence indicates that this ratio is not exceeded. An important factor in the elaboration of the multitude of theories of sex-determination is the approximate equality of the two sexes at birth. Since on the basis of chance alone, any sex-determination "theory," however badly it warps the facts, is sure to be "right" half the time, those who enthusiastically advance such views can generally account for failures of their pet schemes. Thus we can easily understand how it is that several hundred "causes" of sex have been "discovered" and "demonstrated."

consist of boys only, and similarly one should be all girls. If we take families of ten children, even then in every 1,024 families there should be one of boys only and one of girls only. Such large families of children all of one sex attract unusual attention, and the proportion they bear to families more nearly approaching equality is overlooked.

If Unterberger's records of an unbroken series of 74 male births as a result of treatment with sodium bicarbonate could be considered as a random sample from treated individuals the positive effect of the treatment would be practically certain. The probability of obtaining such a result where chance only is operating would be 0.5^{74} or practically zero. We are led to wonder, however, whether Dr. Unterberger's patients have not tended to report favorable results and failed to report those cases which did not give the hoped-for result.

Füth[4] has published some results which, if taken uncritically, may seem to support Unterberger's hypothesis. The vaginal secretion of women was investigated as to its "purity" (bacterial content) during the last month of pregnancy. The kind and number of bacteria in the secretion was taken as an indication of the acidity. It is re-

ported that women with supposedly rather acid secretion gave birth to a preponderance of girls whereas those with slightly acid or neutral reaction gave birth to a preponderance of boys. The numbers are so small, however, that they have little meaning. Furthermore, it can not be assumed that the acidity relations were the same at the time of fertile coitus as one month before parturition, although the author presents some figures tending to show that there is a certain persistency in the acidity of the secretion in the individual woman.

Experiments with Rabbits and Mice

Unterberger and Kirsch[19] have tested Unterberger's method of sex control on rabbits. In one series of experiments the vagina of the does was washed with 0.25 per cent $NaHCO_3$ solution immediately before coitus, and in another series the sodium bicarbonate was administered as powder or mixed with cacao butter. In a third series lactic acid in cacao butter was used. The following results were obtained.*

The sex ratio of the controls is distinctly abnormal and can not very well be used for comparison. According to Mirbt[12] the secondary sex ratio in 715 rabbit litters, comprising 2,353 young, in the Institute of Genetics, Berlin-Dahlem, was 50.96 ± 1.03 per cent males. This figure may, therefore, be considered to represent approximately the normal sex ratio in rabbits. Even using 50.96% males as the basis of comparison the ratios obtained by Unterberger and Kirsch in litters from does treated with $NaHCO_3$ deviate significantly. Litters from the small number of does treated with lactic acid show no clearly significant deviation from the normal sex ratio, but compared to the $NaHCO_3$ group the deviation is significant. The experiments are, however, scarcely extensive enough to verify Unterberger's hypothesis of sex control. That they need to be carried out on a more extensive scale in order to give decisive results is also realized by the authors and they state that the investigations are being continued. They state further that they are treating cows with lactic acid solution. We have, however, as yet seen no report of the results of this treatment.

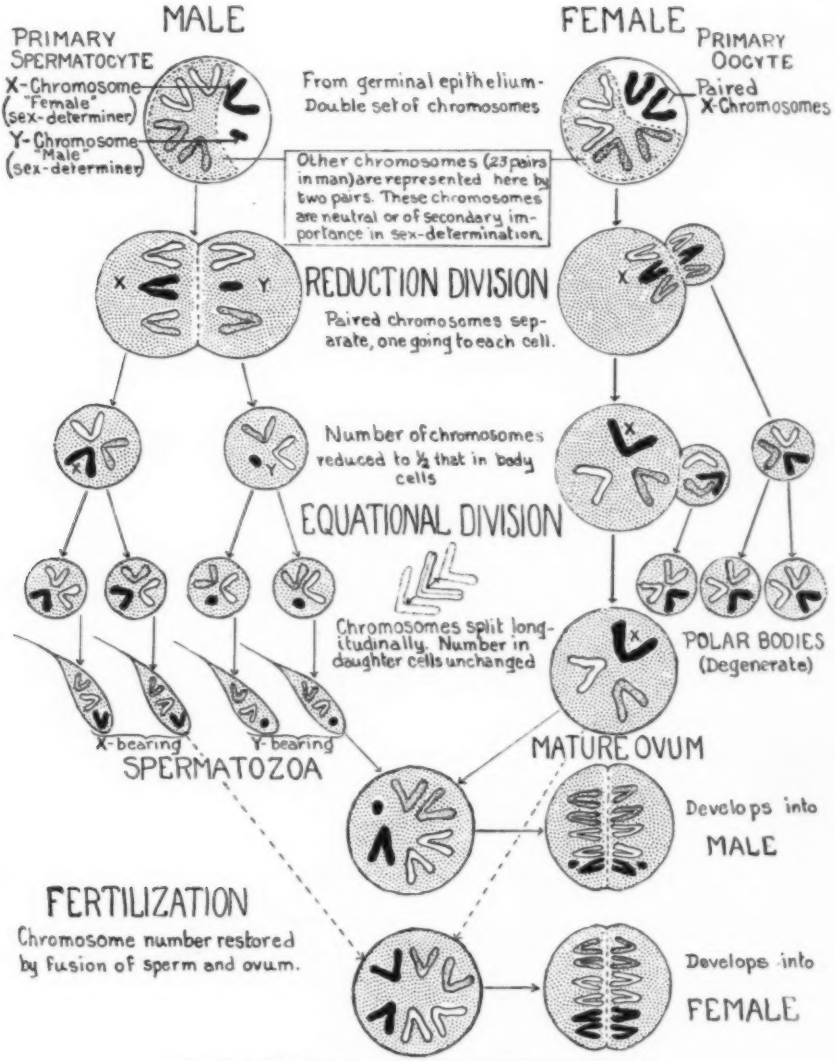
Bluhm[1] has injected a 0.25 per cent $NaHCO_3$ solution into the uterus and vagina of mice before copulation. The solution could be injected into only one uterine horn, however, due to contraction of the uterus as a result of the treatment. The following results were obtained.†

The difference in sex ratio between the experimental and the control groups is 4.24±1.635, or 2.59 times its standard error. Miss Bluhm considers this difference significant, "since all other conditions that might have influenced the sex ratio were excluded;" but although litter mates of the same inbred stock were used both for treatment and control, this does not guarantee that all other factors than the hydrogen ion

*Results with Rabbits	Total No. of Young born	♂♂	♀♀	%	♂♂
Control	84	28	56	33.33 ±	5.143‡
Treated with 0.25% $NaHCO_3$	121	80	41	66.12 ±	4.303
Treated with $NaHCO_3$ as powder or in cacao butter	76	55	21	73.37 ±	5.129
Treated with lactic acid	21	5	16	23.81 ±	9.294

‡The standard errors are calculated by the present authors.

†Bluhm's Results	Av. Litter Size	♂♂	♀♀	Total No. of Young	Sex Ratio %	♂♂
Injected	5.01	859	1037	1896	45.30 ±	1.143
Control	4.91	913	930	1843	49.54 ±	1.168


MALE
FEMALE
PRIMARY SPERMATOCYTE
X-Chromosome "Female" (sex-determiner)
Y-Chromosome "Male" (sex-determiner)
From germinal epithelium-Double set of chromosomes
PRIMARY OOCYTE
Paired X-Chromosomes
Other chromosomes (23 pairs in man) are represented here by two pairs. These chromosomes are neutral or of secondary importance in sex-determination.
REDUCTION DIVISION
Paired chromosomes separate, one going to each cell.
Number of chromosomes reduced to ½ that in body cells
EQUATIONAL DIVISION
Chromosomes split longitudinally. Number in daughter cells unchanged
POLAR BODIES (Degenerate)
X-bearing
Y-bearing
SPERMATOZOA
MATURE OVUM
Develops into MALE
FERTILIZATION
Chromosome number restored by fusion of sperm and ovum.
Develops into FEMALE

THE MECHANISM OF SEX-DETERMINATION

Figure 2

Sex is normally determined by a pair of chromosomes, the so-called *X* and *Y* chromosomes. These chromosomes have actually been identified under the microscope in many species of animals and their importance in the sex-determining mechanism can hardly be doubted. Such definitely sex-linked characters as color blindness and haemophilia are convincing secondary evidence that this mechanism operates in man as it does in other mammals. If a method can be found for separating spermatozoa bearing *X*-chromosomes from those bearing *Y*-chromosomes the control of sex would become a possibility. As yet, however, no method of separating these two types has been satisfactorily demonstrated.

concentration of vagina and uterus were eliminated. Even if we admit that the small difference between the two groups is significant, which probably is not justified, the effect of the treatment is *in the opposite direction to that obtained by Unterberger.*

In a series of 26 matings lactic acid was used instead of sodium bicarbonate. At first a 0.1 per cent solution of lactic acid was used, but this was found to be too strong since small litters and many abortions resulted. A 0.05 per cent solution subsequently tried seemed to produce no harmful effects, as an average litter size of five was obtained. The sex ratio is reported as 42.30, which is a still smaller proportion of males, but as the exact numbers are not stated the significance of the deviation can not be estimated.

Experiments with Swine

Through the cooperation of the Animal Husbandry Department of this Station we were able in November and December, 1932, to run a limited experiment with swine to test the possible effects of alkalinity of the vagina on the sex ratio. Thirty-two sows were treated, the vagina of each being flushed, immediately before breeding, with 50 cc. of a solution of 0.5 per cent sodium bicarbonate in distilled water (pH approximately 8.0), care being taken that the solution reached the *os uterus* and that enough was injected to wash thoroughly the whole vaginal cavity. Unterberger and Kirsch used a solution of only 0.25 per cent $NaHCO_3$ but this solution was found to be so low in alkalinity that an influence on the reaction of the vaginal secretion could hardly be expected. In a few cases of treatment of women Unterberger had used a 1 per cent solution, but in order to avoid any chance of its having a harmful effect on the spermatozoa only half this strength was used by us. Of the treated sows only 19 conceived after the first mating. Thirty-six other sows in the same herd and bred during the same period were used as controls. For the "normal" sex ratio in pigs, however, we refer to McPhee, Russell and Zeller[11], whose data are included in the table below.

The sex ratio in the treated and non-treated group is practically the same and neither of these differs significantly from the ratio given by McPhee *et al.* Unterberger's method of sex control therefore proved completely ineffective in this case. The results are too meager, however, to have any decisive value.

Theories of Sex Control

The outstanding facts with regard to sex determination and sex control at present known may be summarized rather briefly. It is thoroughly established that in mammals there are regularly two types of spermatozoa (one bearing an X-chromosome and the other the Y), which are normally produced in equal numbers. The eggs are all of one sort, each with an X-chromosome. The sex of an individual depends on whether the egg is fertilized by an X-bearing or a Y-bearing spermatozoön. In the former case the result is a female (XX) and in the latter a male (XY). If only chance operates in the union of spermatozoa and eggs a 50:50 result should ensue, so that the primary sex ratio on this basis should be equality. This primary sex ratio, that is the sex ratio of all eggs fertilized, would naturally be altered if the proportions of X- and Y-sperma-

	No. of litters	No. of pigs	Per cent still born	Average no. of pigs per litter	♂♂	♀♀	Sex ratio % ♂♂
Treated with $NaHCO_3$ sol.	19	170	8.43	8.95	83	87	48.82±3.833
Not treated	36	289	3.81	8.03	141	148	48.79±2.942
McPhee et al.[11]	....	22702	....	....	11883	10819	52.32±0.22

Patented Sept. 5, 1922. **1,428,065**

UNITED STATES PATENT OFFICE.

HUBERT ROYDS TIDSWELL, OF WINNINGTON, ENGLAND.

SEX CALCULATOR.

Application filed March 30, 1920. Serial No. 369,927.

To all whom it may concern:

Be it known that I, HUBERT ROYDS TIDSWELL, a subject of the King of Great Britain and Ireland, residing at 46 Winnington Lane, Winnington, Cheshire, England, have invented certain new and useful Improvements in Sex Calculators; and I do hereby declare the following to be a full, clear, and exact description of the invention, such as will enable others skilled in the art to which it appertains to make and use the same.

A device for finding the sex of all children, born to the same woman, after the first child has been born, when one knows the approximate dates of births of the expected children and the sex of the child previously born. The device also shows when fertilization must take place for a particular sex to be born.

This invention is based on the well-known and proved theory that in normal women the ovaries ovulate alternately, and that one ovary always produces male ova and the other female ova, and that this action occurs at regular intervals, the sequence not being disturbed by gestation or lactation. Consequently if the date of birth and sex of first child born be known, one can predict or determine the sex of subsequent children. The device consists of two or more parts of any regular geometrical form, and

PATENT OFFICE HAS ACCEPTED "SEX-DETERMINATION"

Figure 3

That "the well known and proved theory" on which this calculator is based is a proven fallacy has not prevented this inventor from displaying considerable ingenuity in the development of this device. It is not difficult to cite instances of women with only one ovary producing children of both sexes, and reference may also be made to the birds, which consistently produce the sexes in approximately equal numbers with only one functional ovary.

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tozoa to which the eggs are exposed could be changed. They might conceivably be produced in unequal proportions, though this would require a modification of the known mechanism of their production; the two classes might be artificially separated, partially or completely; or poisons or other chemicals might injure or decrease the motility of one class, which would favor the chances of the ova being fertilized by the other.

The proportion of the sexes in the young at birth is called the secondary sex ratio and it is only of this that we have definite knowledge, though our information may be extended somewhat towards the primary sex ratio by the examination of fetuses. The secondary sex ratio depends in the first place, of course, on the primary ratio, but many causes may operate to effect an elimination of one sex or the other and thus greatly modify the result at birth. If this elimination could be controlled it would constitute a method of sex control so far as the secondary ratio is concerned, but would be more comparable to killing off one sex after the young are born.

Countless theories of sex determination and sex control have been proposed but they have done more credit to the imagination of the proposers

than to their scientific acumen. No attempt will be made to review these, but from what has been said it will be seen that a scientific basis for sex control may really exist, especially with reference to modification of the primary sex ratio. Some of the more recent theories may be mentioned, even if it must be admitted that not all are based on a solid scientific foundation. Leupold[9], according to Mirbt[12], concluded, for example, on the basis of rather limited experiments, that the egg cells of the rabbit became differentiated in male or female direction according to the cholesterol-lecithin content of the blood serum. It is surprising that such an hypothesis, disregarding the whole body of existing scientific evidence, should be proposed on the basis of numerically quite insignificant data. The hypothesis was definitely disproved by Mirbt. To the same class belong some observations made by Siegel[15] in Germany during the war. Siegel kept records of the time of coitus in relation to menstruation and thought he could verify an earlier expressed idea that sex is determined by the degree of maturity of the egg. According to his data, coitus during the first days after the cessation of menstruation resulted in a preponderance of male births whereas coitus between ovulation and the following menstruation gave a preponderance of females. The difference in sex ratio between the groups of children from early and late intermenstrum coitus is statistically significant, but the accuracy and random sampling of the records are open to suspicion.

That the sex ratio may be influenced by the stage of the menstruation interval at which coitus occurs is not inconceivable, since the changes in the female genital tract during this interval might well be such that at one stage the *Y*-bearing and at another the *X*-bearing spermatozoa are favored in their race to the egg. If any form of attraction exists between the ovum and the sperm, which is disputed, it might also be possible that the egg would attract the *Y*-bearing spermatozoa comparatively more strongly immediately after ovulation than later on. Siegel's observations are, however, too fragmentary to be taken seriously. Pearl and Parshley[13] published data for cows tending to show that service late in heat increased the chance for male birth but a later investigation by Pearl[14] on more carefully collected data showed no significant difference in this respect.

Other Attempts at Sex Control

Bluhm[1] injected male mice with alcohol every other day and bred them to untreated females. In 67 litters with 331 young she obtained 54.98 ± 2.734 ♂ ♂. The sex ratio in the control stock was $44.38 \pm 1.299\%$ ♂ ♂, and the difference between the two ratios is statistically significant. The latter ratio is, however, clearly abnormal and can not be accepted as basis of comparison. Miss Bluhm thinks that the low sex ratio is characteristic of the stock and she refers to King's[6] successful selection for high and low sex ratio in an inbreeding experiment with albino rats. Miss King's result, perplexing in itself, can certainly not be extended to verify the low ratio obtained by Bluhm. No other investigation has shown any significant difference in sex ratio between different strains of the same species of mammals or birds, that could not be explained on the basis of differences in the intrauterine mortality. The sex ratio obtained by Miss Bluhm from matings of alcohol-injected males to untreated females does not deviate significantly from the normal sex ratio in mice. The experiment was repeated by Chaudhuri[3], who obtained similar results, but in his case also the sex ratio of the controls was abnormally low and can not safely be used for comparison.

Lush[8], on the strength of a possible difference in the specific gravity of the two kinds of spermatozoa in the rabbit, attempted to separate them by centrifuging and then artificially impregnating unbred females with the different fractions. The method must be admitted as relatively crude considering the small difference in size, if any, that there must be, and it is therefore

***NOT* A ONE-SEX FAMILY**

Figure 4

This photograph of former Governor and Mrs. Richards of South Carolina and their nine daughters was submitted as a striking example of an all-girl family. One need go no further than *Who's Who* to learn that this couple also had two sons which rather spoils the picture as an illustration of a one-sexed family. In families of 11 children there are 67 chances in 2048 (or about 1 in 30) that 9 or more of the children will be girls and of course equal chances that 9 or more will be boys. Thus, purely on a basis of chance, such a preponderance of children of one sex is not phenomenal. Enthusiastic credulity seems to be a prime requisite of many popular pronouncements on sex-determination, and of many "discoverers" of new methods of bringing it about.

perhaps not surprising that the results were negative. It is possible more refined methods may give better results. Thus Koltzoff and Schröder[7] have tried to separate the *Y*- and *X*-bearing spermatozoa by passing an electrical current through a sperm suspension. Rabbits were used for the experiments. One doe artificially inseminated by sperm from the anode produced six young, all of the female sex. Another doe was impregnated with sperm from the cathode, and produced four male and one female young, while a third doe impregnated with sperm from the central part of the tube produced two male and two female young. These results are very meager but the authors consider it improbable that they were due to chance alone. Repetition of the experiment on a larger scale is greatly to be desired.

Conclusion

The sex ratio in man and domestic animals is subject to considerable variation and very little is known as to the causes of these variations. For the most part they are so numerous and complex that we simply have to characterize the variation as depending on chance. A small sample may often show an unusually high, or low, sex ratio, which may even have a deviation of slight statistical significance from the "normal" ratio. But when fairly large samples are taken *at random* the sex ratio in all species shows a surprising stability and a closer examination of the data reveals, as a rule, that differences between sub-groups can be attributed to chance. Where significant differences are found, a different rate of intrauterine mortality is usually the cause[5]. There is, of course, no *a*

priori reason against the possibility that the relative acidity of the female generative tract may have a differential influence on the motility of the two types of spermatozoa and thus effect a change in the sex ratio; the question is whether it is at present a demonstrated fact. If Unterberger's reported results are accepted at their face value there would appear to be no avoiding the conclusion that in man, at any rate, sex can be controlled by this means with a high degree of certainty. There are, however, a number of biological reasons that urge caution in reaching this conclusion, and further confirmation will doubtless be awaited, at least in biological circles, before it will be generally accepted. This is all the more true in view of the generally inconclusive outcome of attempts to reproduce the results in experiments with animals. It is probably best to conclude, therefore, that up to the present time all claims that the sex ratio in mammals can be controlled are premature and for that reason they have often done science more harm than good, since they are apt to make the layman suspicious of the validity of the results of strictly scientific investigations as well. It is quite possible that the time will come when geneticists will be able to separate male- and female-determining spermatozoa and thus to a corresponding degree control the sex ratio, but so far no definitely positive results have been obtained. Even if the relative acidity of the vagina should conceivably have an influence in determining the sex of the offspring, there is no basis for assuming that this can be controlled by diet.

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EDUCATION AND SIZE OF FAMILY

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DIFFERENTIAL fertility has been the subject of numerous research projects in the United States, especially during the last decade. A conspicuous rôle in bringing about differences in the size of families has been attributed by most investigators to the factor of education. This is attested by the fact that thirteen articles have appeared on this topic in the JOURNAL OF HEREDITY alone during the last ten years, not to mention the studies found in other Journals, as well as in separate monographs.

The studies may be classified roughly into two principal groups. In the first group there is the large number of investigations which conclude that education is an important causal factor in, if not the principal explanation for, the decline of the birth rate.* In the second group are to be found the few articles which question the importance of education *per se* as a causal factor and suggest that we look elsewhere for the explanation. This latter view is expressed by Banker[1]: "It is the writer's opinion that education, and especially formal education has little or nothing to do with the declining birth rate, that the correlations between education and fecundity which have been so greatly exploited are spurious or rather that they are simply mutual correlations with a *tertium quid* and are not directly related."

Most of the literature on this problem is based on data taken from the families of students or alumni of Harvard, Yale, Dartmouth, Vassar, Wellesley, Oberlin, and other institutions. They therefore represent a selected group and can hardly be said to reflect the actual conditions in the population at large. Studies by Holmes[4] were concerned with the families of the parents of college students and are therefore somewhat more representative of the general population than those based on the families of college students themselves. Professors N. I. Butt and Lowry Nelson[2] made a study of two entire Utah villages and included all classes as far as represented in these villages. They found that education had very little, if any, influence on the size of the family. It is possible, however, that the nature of the Mormon religion counteracted any influence which may have been present otherwise.†

Source of Data

The material in the present paper may be of interest because of the fact that it is taken from rural families in various parts of the State of Connecticut. It is a segment of a larger project‡, the data for which were collected by the Department of Sociology at the Storrs Agricultural Experiment Station in the fall and winter of 1929 by means of a house-to-house survey of 616 rural families

*Few attempts have been made to explain exactly how this factor operates, but the implications suggest that it is through the channels of birth control and postponement of marriage.

†The church plays the leading part in the social organization of the Utah villages, and Mormon philosophy idealizes the large family. For a discussion regarding the influences of Mormonism on the size of the family see Thompson[10] (pp. 135-136 ff.). A more extreme case showing the possible counteracting influence of religion is the study by Griffing[4] (pp. 331-337) where education was found to result in an increase rather than a decrease, in the size of family.

‡This will be published in the near future as a bulletin on "Population Mobility in Rural Connecticut."

in six towns* selected because of type of farming, nearness to the city, and other factors. The selection of these families was made with the object of obtaining as nearly as possible a true cross-section of rural life of the state.

The data for the present paper are confined to those husbands and wives of completed families; i. e., families in which the wife and mother is 44 years of age and older. Exceptions were made of a few cases where the mother was between the ages 40 and 44, but whose youngest child was at least ten years of age. These parents (husbands and wives) were then classified according to two characteristics: the number of years schooling received, and the number of living children.† The results are presented in Table I.‡ The mean number of children was 3.2, while the mean number of years' schooling was 7.8 years. The coefficient of correlation between number of children and number of years' schooling was $(-.30 \pm .025)$, a rather low but possibly significant negative correlation.

A perusal of Table I shows a tendency for cases to cluster in the column showing eight years of schooling. Since a great many of these parents were foreign born§, it was rather difficult in many instances to ascertain the exact amount of schooling. There may have been a slight tendency for individuals to report an elementary education (8 years), when less than this amount was actually received. The writer is of the opinion that whatever error might result from this tendency would be mini-

TABLE I.—Parents Classified According to Education and Number of Living Children.

No. of years schooling	Number of Children														Total No. of parents
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	
15 & over	2	4	....	....	1	....	....	....	....	....	....	....	....	....	7
14	3	1	3	....	....	....	....	1	....	....	....	....	....	....	8
13	....	1	1	1	....	1	....	....	....	....	....	....	....	....	4
12	15	6	14	11	9	5	3	2	2	....	....	....	....	....	67
11	4	4	....	4	2	....	....	....	....	....	....	....	....	....	14
10	11	5	6	8	2	1	....	1	....	....	1	....	....	....	35
9	....	7	5	3	3	1	....	2	....	....	....	....	....	....	35
8	43	39	55	57	30	21	16	10	10	8	2	2	....	1	294
7	8	2	6	7	5	....	2	....	....	1	....	....	....	....	31
6	8	3	5	1	3	8	1	7	1	3	....	....	....	....	40
5	4	2	6	2	....	1	....	1	2	2	....	....	....	....	20
4	2	....	1	1	3	....	....	....	....	....	....	....	....	....	7
3	....	....	2	2	2	....	3	1	1	....	....	....	....	....	11
2	....	1	1	1	2	....	2	....	....	....	1	....	1	2	11
1	....	....	1	1	3	1	2	....	....	....	2	....	....	....	10
0	2	....	2	3	4	6	6	....	3	....	....	2	1	....	29
Total Number of Parents	102	75	108	102	69	45	35	25	19	14	6	4	2	3	609

*"Town" in New England corresponds to township elsewhere in the United States.

†No information was secured regarding the deceased children. The absolute size of the family will therefore appear smaller than in other studies where total number of children born was enumerated.

‡The number of children in each family was therefore entered twice; i. e., first according to the education of the mother, and second, according to the education of the father.

§The United States Census for 1930 shows that only 42 per cent of the Rural-Farm population of Connecticut are native-born of native parentage, while 22.3 per cent are foreign born, 28.5 per cent are native born of foreign parentage and 7.2 per cent are native-born of mixed parentage. See the data in the article by Hypes[6] (pp. 189-205).

mized by lumping the first eight years into one group. The probabilities of ascertaining whether an individual belonged to the elementary group (0 to 8 years), the high school group (9 to 12 years), or the college group (more than 12 years), would be much greater than ascertaining the exact number of years. These three categories are therefore used in Table II. The relationship is again inverse. Whereas the parents with only an elementary education or less, averaged 3.5 children, the high school averaged only 2.4 and the college parents only 1.7 children.

Table III shows the average number of children per family according to biparental education. In order to increase the number of cases, only two categories of education are used. "Elementary" includes zero to eight years; "Beyond elementary" includes nine years or more.

The average number of children in families where neither parent has had more than an elementary education is 3.7 as compared with 2.9 where the father alone has gone beyond the elementary school, and 2.8, where the mother alone has more than an elementary education. Where both parents have more than eight years of schooling the average number of children per family is reduced to 2.0. The difference appears to be consistent and significant.

Effect of Religion and Occupation

If we classify these same parents according to religious affiliation we find that the size of the family likewise varies according to whether the parents are Protestants or Catholics. There were 144 Catholic parents in the sample; the average number of children per family was 4.6, as compared to an average of 2.6 for the 400 parents professing some variation of the Protestant belief.* In a similar manner, if they are classified according to whether foreign or native born, we find that the 331 parents born in the United States have an average number of 2.48 children. The 60 parents one of whom in each couple was foreign and the other native born, had an average of 3.3 children. The 208 cases where both parents were foreign born averaged 4.1 children per family.

That occupation is likewise a factor influencing the size of the family is shown by the fact that when classified according to whether the fathers were engaged in farming occupations or non-farming occupations, the 388 farm parents had an average of 3.7 children as compared to 2.3 for the non-farm parents.†

The above data lead one to inquire

TABLE III.—Average Number of Children Per Family According to the Degree of Education of the Parents.

Mothers	Fathers: Elementary	Fathers: Beyond elementary
Elementary	3.7	2.9
Beyond elementary	2.8	2.0

TABLE II—Parents Classified According to Education and Average Number of Children

	No. of parents	Average no. of children
Elementary or less	453	3.5
High School	137	2.4
College	19	1.7

* Some of the cases were omitted because of the lack of information on this point.

† For further information regarding occupation and size of the family, see Sorokin and Zimmerman[5]. Also compare Sims,[6] p. 247 ff.

whether the observed differences in the size of the family, attributed to differences in formal education, may not be merely the result of a spurious relationship which is to be explained in terms of religious belief, occupation, social and economic conditions existing in foreign countries and reflected in the attitudes and customs of the foreign born parents, contacts and influences radiating from urban environments*, and other conditions.

Table IV is an attempt to show the net result of formal education on size of the family when national origin, religion, and occupation are eliminated as factors. This is done by omitting all Catholics, foreign born, and parents engaged in occupations other than farming from the classification. This procedure reduces the size of the sample to 177 parents, and therefore must be taken as suggestive rather than conclusive, pending additional investigation. For the native born, Protestant, farm parents having only an elementary education or less, the net average is 2.64 children per family. For those having more than an elementary education, the average is 2.65 children. The difference which was significant when in conjunction with other factors disappears when isolated from them.

It would seem, therefore, that the phenomenon of differential fertility, as found among the rural population, is the result of several factors in combination, and that the amount of formal schooling, by itself, has little, if any, influence as yet on the size of the family.

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TABLE IV.—Native Born, Protestant, Farm Parents Classified According to Education and Number of Children

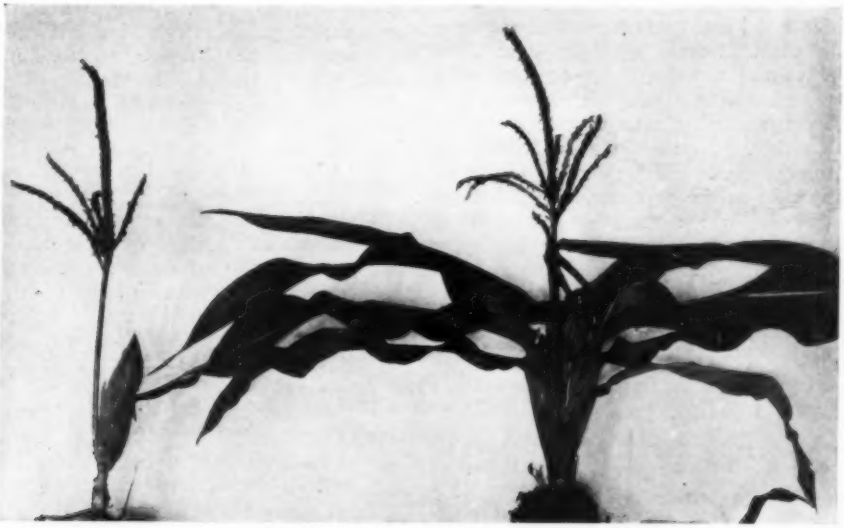
Education	0	1	2	3	4	5	6	7	8	9	10	11	Total	Aver.
Elementary education or less	24	19	21	24	11	8	6	1	3	1	....	2	120	2.64
Beyond elementary education	9	6	13	15	6	4	....	2	2	....	....	....	57	2.65
Total	33	25	34	39	17	12	6	3	5	1	....	2	177	2.63

* See the study by Thompson[10] for a discussion of the influence of urbanization. Also see the discussion in Sorokin, Zimmerman, and Galpin[8] for Rural Sociology.

HERITABLE CHARACTERS IN MAIZE

XLV—Nana*

H. W. Li



NANA PLANTS AT VEGETATIVE MATURITY

Figure 5

The leaves have been stripped from the plant on the left to show the very short internodes. The final internode is of nearly normal development and it alone accounts for as much as three-quarters of the total length of the plant.

HUTCHISON† published genetic data involving a dwarf type of maize called nana (*Na na*). This drawf type was found in a field of corn, *Cornell 12*, by Professor H. H. Love, of Cornell University. The character was found to behave as a monogenic recessive. Dr. I. F. Phipps made certain crosses between nana and various linkage tester stocks in order to determine the linkage relations of the *na* gene. This material was turned over to the writer in 1927.

Description of Nana

Nana, in the mature stage (Figure 5), is intermediate between a dwarf and a semi-dwarf. It usually ranges from one foot to two feet in height. It is characterized by the shortening of the internodes throughout the culm except the last one bearing the tassel. The number of internodes is normal. The last internode is very long compared with the rest. It comprises about one-half to three-fourths or more of the length of the whole plant. The leaves are longer than those of the dwarfs, but shorter than those of normal plants. The leaves exhibit a stiffened and rigid appearance due to their thickened condition. In most cases the leaves are characteristically twisted. The culm is smaller in diameter than normal, but with the bunched condition of the sheaths at the base of the plant,

*Paper No. 173, Department of Plant Breeding, Cornell University, Ithaca, N. Y.

†C. B. Hutchison, Cornell Agr. Exp. Sta. Mem. 60. 1922.

the culm actually looks larger than the normal sib plants. The leaf sheaths are extended much beyond the internodes above them, as can be seen in the accompanying plate. The tassel is normal. The ears are as a rule slender and long. One to several ears can be obtained from a single plant. Anthers are not found in ears of nana plants. Nana can be distinguished from normal plants very early after planting, generally in about twenty to thirty days. When grown in sand benches in the greenhouse in the usual way in which material for seedling counts is grown, the plants do not make enough growth to make differentiation of dwarfs and normals clear cut. However, on growing the seeds in flats of good soil for about twenty days or more, the separation can be made with a high degree of certainty. Nana seedlings are characterized by a shorter stature than the normals, ranging from one-half to two-thirds the height of the normals. The culms are thicker than those of the normals. The leaves are shorter with characteristically twisted blades. When held in the hands they have a more rigid and stiffer feeling than the normal seedlings.

In the seedling stage, nana segregates in good agreement with the theoretical expectancy. In the field, however, the poor viability of nana plants is so apparent that it makes the study of the linkage relations somewhat difficult.

Linkage Relations of Nana

Hutchinson reported that nana is independent of *sh*. In the repulsion phase he obtained 251 parental types and 248 new types, almost a perfect agreement. Suttle* also reported counts on nana with shrunken, giving 313 parental combinations and 281 new combinations in the coupling phase. Suttle also tested nana with liguleless and obtained 312 parental and 283 new combinations, indicating independent inheritance. This earlier work is all confirmed by the present writer.

As has already been stated, nana plants are less viable in the field than the normals. Therefore, it is difficult to determine the linkage relationship from the F_2 results. In testing with *A a* the following results were obtained:

Na		*na*	
A	*a*	*A*	*a*
167	64	46	6

These data indicate a recombination value of about 35.5 per cent as calculated by the use of Immer's tables.† However, they do not deviate widely from a normal dihybrid ratio. Back-cross data as shown in Table I demonstrate clearly that nana belongs to the *A* ts_4 group.

Dr. D. F. Jones, of the Connecticut Agricultural Experiment Station, kindly wrote the writer giving results he obtained in back-crosses involving *na* and *A* in the coupling phase. He obtained 126 of the parental combinations and 90 of the new combinations. This gives a recombination value of 41.7 per cent. These, together with the results obtained by the writer, show the distance between *A* and *na* to be such as to give about 36 per cent of recombinations.

In the F_2 generation of the cross *a* ts_4 *Na* × *A* Ts_4 *na* the following classification was made:

Na		*na*	
Ts_4	ts_4	Ts_4	ts_4
58	32	19	0

Two of the plants classified as *Na* ts_4 may have actually been *na* ts_4. The data suggest that *na* and ts_4 may be closely linked, in which case one would expect the order of the genes to be *A*—*na*—ts_4. More reliable data must be obtained in order to decide this point.

*A. D. Suttle. Unpublished Thesis, Cornell University. 1924.

†Immer, F. R., *Genetics* 15:81-98, 1930.

Tests of Nana with Other Linkage Groups

A summary of linkage tests of nana with factors in the linkage groups believed to be independent of the *A—ts*$_4$ group are given in Table II. There is no good indication of linkage with any of these genes. In the case of tests with *C, sh, wx* and *f*, the observed ratios deviate rather widely from expectancy due to poor germination and poor viability.

Summary

Nana, a recessive factor for dwarf plant, was found to be in the *A—ts*$_4$ chromosome, possibly located between these two factors. The recombination percentage of *A* and *na* was 35.5 ± 0.87. Nana was found to be independent of *C, sh, wx, R, g, su, Tu, B, lg, Y, Pl, f, ts, ra, gl, pr*, v_3, *bv* and d_1.

TABLE I. Backcross $\frac{A\ Na}{A\ na} \times a\ na.$

Pedigree number	Parental combinations		Rec combinations		Total	Recombinations	
	A Na	A na	A Na	a na		Number	Per cent
1042-1 x 758	73	69	64	40	246	64	26.5
516-1 x 4768	30	42	13	18	103	31	30.1
516-3 x 4783	30	43	25	20	130	45	34.6
516-5 x 4769	47	45	21	27	140	48	34.3
516-5 x 5069-2	57	53	29	20	159	59	37.9
516-9 x 5063-2	44	49	20	24	137	44	32.1
516-11 x 4768	30	29	27	15	101	42	41.6
517-12 x 4768	33	55	17	26	131	43	32.8
Subtotal	355	365	196	199	1113		
Total	720		395		1119	395	35.3

TABLE II. Summary of the tests of Nana factors with factors in other linkage groups.

Genes involved	Phase	Number of individuals: Na		na		Parental combination	Rec combination	D/PD	P#
		I	II	I	II				
	Repulsion	237	57	68	14				0.0009
	Repulsion	125	26	45	10				0.06
	Repulsion	46	20	16	13				0.65
	Repulsion	479	543	499	493	1022	992		
	Coupling	380	394	328	320	678	656		
Total						1700	1688	1.0	
	Coupling	409	415	338	345	774	784		
	Repulsion	72	81	37	41	116	113		
Total						890	869	0.7	
	Repulsion	101	57	98	57	155	158	0.3	
	Coupling	16	45	7	14	30	52	3.6	
	Coupling	178	169	122	116	294	291	0.2	
	Repulsion	160	131	159	115	270	275	0.3	
	Coupling	84	53	31	28	80	84	1.0	
	Coupling	177	169	129	116	295	298	0.2	
	Repulsion	430	101	134	88				0.0005
	Repulsion	81	98	91	43	123	194	0.9	
	Repulsion	73	71	68	88	174	183	0.5	
	Repulsion	80	76	80	77	136	197	1.8	
	Repulsion	113	100	109	98	209	211	0.1	
	Repulsion	17	4	2	0				
	Repulsion	296		239	169				0.07**

P is given in the case of F_2 population.

**Calculated on the basis of an expected 3:3-2 ratio. The *Na d* and *na* d_1 classes are lumped together in the *na* d_1 column.

"Mongolian" Twins

That complex of characters which goes by the name of Mongolian idiocy has caused no end of controversy among members of the medical profession and students of human genetics. One school of thought holds that the condition is due to "uterine exhaustion" and statistics have been compiled to prove that Mongolism is much more frequent in the higher birth ranks and among the children of older women. Statistics proving the reverse have also been submitted in rebuttal, and to prove several other "causes." It is evident that a complete understanding of the causes demands a more refined use of biometrical methods, or an approach from some other quarter.

Mongolism in twins may throw further light on the problem but again the data, or their interpretation, seem to give answers as ambiguous as those of the Delphic oracle. Whether this ambiguity is due to the twins or to the reasoning that is applied to them remains to be seen. One suspects the latter is nearer the truth.

In the *Lancet* (London) April 15, 1933, P. M. G. Russell reports a new case of mongolism in twins. Both twins were girls, and both were Mongolian idiots. Russell feels that their

alleged non-identity thoroughly disposes of the genetic basis of Mongolism. There are at least two serious defects in this reasoning. In the first place the evidence presented that the twins were non-identical is not conclusive. It is based chiefly on the condition of the foetal membranes, and several cases have been published in this Journal and elsewhere in which twins having separate birth membranes have given striking evidence of being identical.

One of these twins subsequently died at the age of five months, and data can no longer be obtained with regard to such important diagnostic characteristics as fingerprints. Even were it admitted that the appearance of a pair of non-identical twins both Mongolians, would forever dispose of a genetic basis for Mongolism, one could hardly accept this case as being conclusive on the evidence presented regarding the non-identity of the twins. The fact that the twins were both girls and that they were both Mongolian idiots, had identical birth-weights, and nearly the same head-circumferences, seems to be a fair basis for maintaining that they *may have been* identical twins in spite of the membrane evidence. The death of one of them before the publication of Russell's report renders this case a closed book.

The reasoning which maintains that the occurrence, in a fraternity, of two individuals showing a similar defect is conclusive evidence that the defect is *not hereditary*, is, to put it mildly, rather unusual. Non-identical twins differ or resemble each other to the same extent that ordinary brothers and sisters do. When the parents are similar in appearance, there is an excellent chance that their children will resemble each other in many characteristics, *but this is not evidence that such characteristics are not inherited.* In the case of any characteristic inherited according to the Mendelian scheme, there is a definitely predictable chance that the characteristic will appear in two consecutive members of a family. If Mongolism is caused by a recessive gene carried by two normal parents, the chances are one in four that any given child will be a Mongolian idiot. If two recessive factors are involved there is one chance in sixty-four that a given individual will be a Mongolian idiot. This is perhaps a slight chance but it is not, as Russell says, "infinitesimally slight."

If we use the word "defect" in a broad sense, there is probably truth in this author's view that "the condition seems to be a summation of physical and mental defects which when present in varying degree, may produce anything from an intelligent child with mongoloid stigmata to a Mongol Idiot." How these defects are to be accounted for, and why, when their appearance so strongly suggests typical Mendelian segregation it is considered necessary to drag in other explanations really less satisfactory remains one of the interesting problems of Mongolism.

The Anatomy of Development

GRUNDZUGE EINER KONSTITUTIONS-ANATOMIE, by Walter Brandt. Pp. 382. RM 28. Julius Springer, Berlin, 1931.

Development, growth, and differentiation have become of first interest to geneticists during the last decade. Here is a book, with extensive bibliographies and 135 illustrations, that considers them in relation to morphology, both plant and animal. Progress in this field is being made so rapidly that no author can keep fully in touch with it, but this comprehensive work should prove useful to anyone who wants a broad survey of the field from the point of view of an anatomist.

PAUL POPENOE.

RECESSIVE COLORATION IN DUTCH BELTED CATTLE

R. B. Becker

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A BRIEF study of inheritance of coat colors in Dutch Belted cattle was made possible from matings of a cow of this breed with a registered Jersey bull at the Florida station. The cow (University's Harvest Queen 3309) was phenotypically black with a wide white belt, black switch, muzzle and tongue, as shown in Figure 6*A*. Nothing is known of the color markings of her ancestors other than that to be registered, it was necessary for them to possess the black coat and white belt typical of this breed.

The Jersey bull with whom she was mated (Fontaine's Wexford Noble 262070) was solid dark fawn, with black tongue, and switch. His sire had white on the brisket, right shoulder and stifle, belly, flanks and legs, a mottled tongue, black and white switch. The dam of Fontaine was solid colored, with black tongue and switch, but carried the recessive factor for white spotting, since two of her progeny, including a full sister of Fontaine, had white markings.

Fontaine's Wexford Noble, himself, carries the recessive factor for white spotting, having inherited this from his sire. However, he shows the dominant characters, solid color, black tongue and switch, inherited from his dam. Proof of this statement is shown by his purebred Jersey progeny born in the Florida station herd, as listed in Figure 7. Examination of this chart discloses the colors of his Jersey progeny to be as follows:

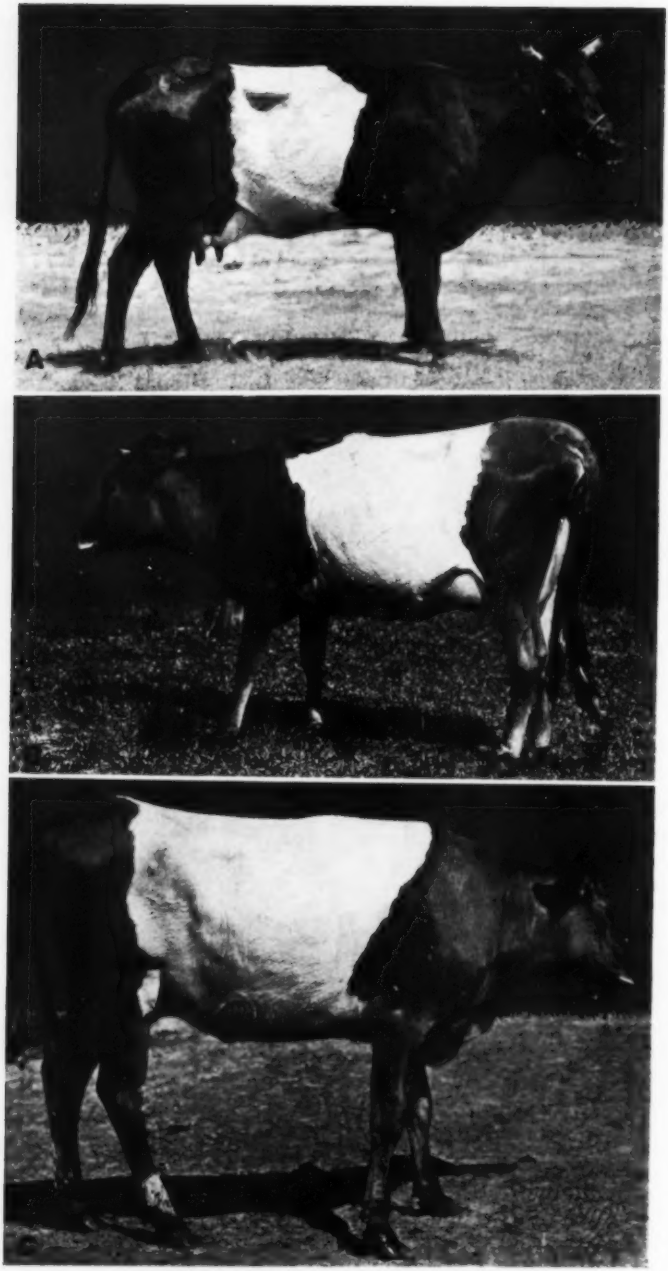
Exact data were not kept of the first and second progeny of the cow, University's Harvest Queen 3309, because of their crossbred character (not eligible to registration), and because their genetic value was not realized by those then in charge. The third, fourth and fifth calves were sired by the Jersey bull whose transmitting ability has just been analysed. The third—heifer No. 393, shown in Figure 6*B*—was red in color, with a wide white belt, small white spots on the front of both hind pasterns, a red switch with white tip, black muzzle and tongue, and a light fillet. The fourth calf—heifer No. 427, in Figure 6*C*—was also red with a wide white belt. The red shaded into cinnamon brown just above each hoof. The tip of the switch was white, and there was a large white spot on the right hind pastern. Muzzle and tongue were black. Rather than being an extension of a wide white belt, in that they did not extend down to the hoofs, the white spots on the pasterns of these heifers, distinctly suggested the Jersey type of spotting.

The fifth calf was born prematurely on the 206th day of gestation. An attendant noted the color markings, but failed to observe the sex, and burned the carcass in the incinerator as soon as it was found. This foetus was black with a white belt and white hind pasterns, black muzzle, switch and tongue.

From the color transmission observed, it is possible to state that

Color of dams	*Color of progeny*	*Male*	*Female*
Solid color, pure*	Solid color*	1	3
Solid color, recessive	Solid color*	3	3
Solid color, recessive	Broken color	4	3
Broken color	Solid, heterozygous	2	1
Broken color	Broken color	5	3

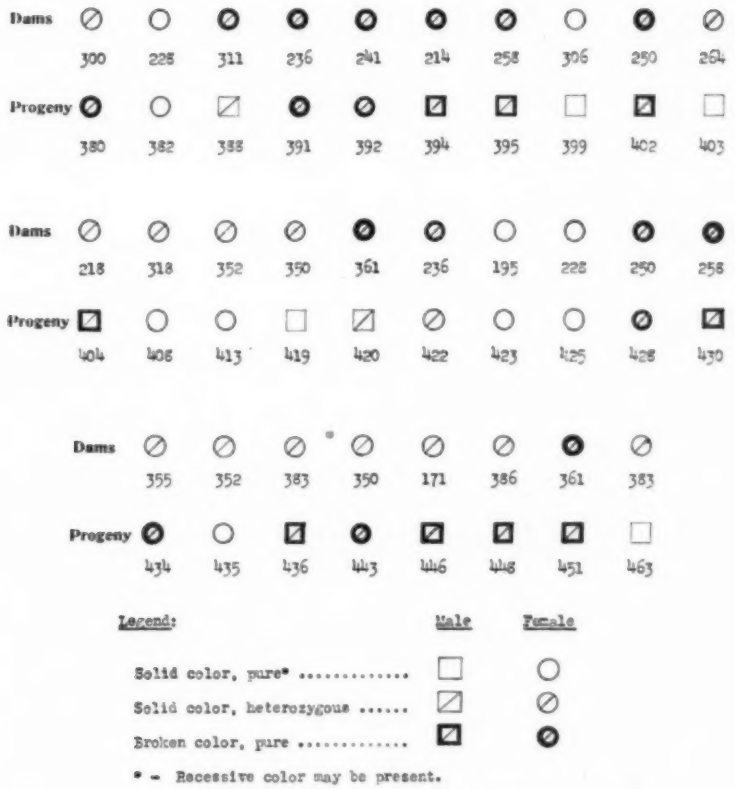
*Recessive factor for broken color may be present in some.



INHERITANCE OF DUTCH BELT

Figure 6

A—University's Harvest Queen 309—a registered Dutch Belted cow. *B* and *C* heifers 393 and 427. Both are the daughters of University's Harvest Queen by a pure bred Jersey Bull, Fontaine's Wexford Noble 362070, who carried a recessive factor for white spotting. Both heifers are *red*, with a white belt.



INHERITANCE OF SPOTTING

Figure 7

The chart above shows transmission of coat color when pure bred Jersey cows were mated with the solid color Jersey bull Fontaine's Wexford Noble 262070.

individuals within the Dutch Belted breed of dairy cattle carry the recessive factor for red coat, and for white spotting. The white belt pattern is probably dominant over the Jersey factor for solid colored body, or at least exhibits itself fully in the presence of the recessive factor for white spotting.

Kuiper[2] observed crosses between the Lakenvelder (Dutch Belted) cattle and the pied breed known as Holstein-Friesian, and concluded that the belted character was dominant to solid or self coloration, and red recessive to black in the Dutch Belted breed. Four of the Dutch Belted cattle illustrated in that report possessed white markings above the hoofs. He assumed that white belt and self color each were governed by a single pair of factors, and stated further:

> Experience in breeding Dutch Belted cattle shows that the calves born are not always belted but sometimes self-colored. In this case they are mostly coal-black, but occasionally when both parents are heterozygous as regards black hair, plain red calves are produced. In the pure breeding of red belted cattle, some cases are positively known in which self-colored calves, in the present instance naturally red, were produced. . . . .

Modifiers seem to play a part in the extent of the white belt. . . . . I have found a very high correlation, viz., 0.84, exists between the occurrence of white feet and too broad a belt . . . (the data having been obtained from illustrations in the Dutch Belted Cattle Bulletin and Live Stock Journal published by the Dutch Belted Cattle Association of America).

This analysis of a Dutch Belted × Jersey cross adds Dutch Belted cattle to the list of black, or black and white, breeds possessing the recessive red coat color character, as mentioned by Cole[1], namely, the Aberdeen-Angus, Dexter, Galloway, Holstein-Friesian and Kerry breeds, and confirms the earlier observations of Kuiper. Dutch Belted cattle may be compared, therefore, as to color transmission, with crosses such as Jersey × Aberdeen-Angus, and Jersey × Holstein-Friesian, as conducted by Pearl, Gowan and Cole.

Summary

Black coat color of Dutch Belted cattle is dominant to Jersey red (fawn).

White belt is dominant to the Jersey characters for solid color, or at least exhibits itself fully in the presence of recessive white spotting.

Recessive white spotting of the Jersey breed appears to be dominant over recessive red of the Dutch Belted cattle.

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2. Kuiper, K., 1921. Color inheritance in cattle. *Jour. Hered.* 12:102-109.

3. ————————, 1920. Onderzoekingen over kleur en teekening bij runden. Naar experimenten van R. Houwink Hzn. *Genetica* 2:137-161.

Plant Breeding Up to Date

RECENT ADVANCES IN AGRICULTURAL PLANT BREEDING, *by* H. Hunter, Hon. M. A. (Cantab.) D. Sc. (Leeds) Plant Breeding Institute, School of Agriculture, Cambridge, and H. Martin Leake, M. A. Sc. D. (Cantab.) Formerly Director of Agri., United Provinces, India, and Principal of the Imperial College of Tropical Agriculture, Trinidad; with a foreword by Sir Rowland H. Biffen, M. A., F. R. S., with 16 plates, Two Parts. Seventeen Chapters. 361 Pages. Price $4.00. P. Blakiston's Son & Co., Philadelphia.

THE book is one of a series of summaries of advances in various biological sciences published originally by J. and A. Churchill, London and now appearing in an American edition. In the words of the authors "we have accordingly attempted to present in a connected form the results of only such investigations as have advanced beyond the limits of purely academic interest and the outcome of which have been the introduction of improved varieties into general agricultural use."

The book contains an interesting foreword by Sir R. H. Biffen. The presentation is in two parts: Part I. Crops of Temperate Regions; Part II, Crops of the Subtropical and Tropical Regions. In Part I the small grains, flax, potato, forage grasses, clovers and root crops are each treated separately. Part II includes beverage crops, sugar cane, tropical fruits, tobacco, drug plants, the tropical cereals, rubber, fiber crops and oil plants. The second part will be of greatest interest to American readers as a connected story of the breeding results with many of these crops has not heretofore been available.

The separate chapters on wheat and oats contain results obtained in Great Britain, Europe and America. The importance and progress in breeding disease resistant varieties is emphasized. The results with barley contain chiefly a summary of breeding for malting purposes in Denmark and the British Isles. The writers apparently do not know that six rowed barleys are preferred by maltsters in the United States. The development of flax varieties for seed and fiber purposes is treated in an interesting

manner. The chapter on potatoes discusses their probable origin and gives particular emphasis to studies of disease resistance. A brief summary of studies of selection in self fertilized lines carried on in the United States, as a means of obtaining parental material of known genetic characters, would have seemed desirable to the reviewer. The chapter on forage grasses reviews the work of the Welsh Plant Breeding Station at Aberystwyth with Perennial rye grass and Cocksfoot. The results given well illustrate the possibilities of improvement with this class of crops. The interesting results with timothy obtained at the New York State College of Agriculture and started many years ago might well have been mentioned. The chapter on clovers including red clover, *Trifolium pratense* L., and white, *Trifolium repens* L., summarizes knowledge of the various type and strain characteristics and briefly outlines the difficulties for the breeder resulting from high self sterility which Williams at Aberystwyth has observed to be slightly greater in red than in white clover. The comparative value of roots and barley as food for livestock introduces the chapter on mangels, Swedes and turnips. These studies probably had as their result the great expansion in Denmark of the area devoted to these root crops, from 6,000 acres in 1861 to 678,000 in 1919.

Tropical Plants

The first group of plants included in Part II are those used in beverages: coffee, cacao, and tea. Brief descriptions are given of the species and varieties used, methods of culture and breeding methods. Because of the great heterozygosity of the commercial varieties, the problems of breeding are many. While considerable progress has been made in improving yield and in controlling diseases, the unsolved problems are numerous and the possibilities of further improvements are very great. The difficulties of breeding improved varieties of sugar cane are due to the heterozygous condition and the lack of genetic knowledge of the mode of inheritance of economic characters. The present status of the problem of breeding is interestingly summarized. Economic species of Citrus and Musa are described and the present status of breeding problems outlined. The heterozygous condition of these crops is emphasized. Species and varieties of the tobacco of commerce and of the opium poppy are described and breeding problems briefly reviewed.

The tropical cereals, maize, rice and sorghums are included in one chapter. Modern methods of maize breeding and the utilization of hybrid vigor are discussed and illustrated. In an adequate treatment of the problem it would seem that a brief mention of the importance of Richey's method of convergent improvement should have been included. With rice and sorghums an outline of their importance, descriptions of species and varieties and their adaptations, modes of inheritance of economic characters and breeding problems are included. The chapter on rubber is concerned only with the Para rubber tree, *Hevea braziliensis*, Muell, Arg. Fiber plants, cotton, sisal, New Zealand flax and Jute are included in one chapter and oil plants, cocoanut, oil palm, castor, ground nut, sesame and soybean, are each briefly discussed in the concluding chapter.

Reference to the literature given at the end of each chapter will be of value to the research worker. The book should be of interest to all students of natural sciences and to commercial growers. It will serve to emphasize the great importance of modern studies of crop improvement. To the technically trained plant breeder the book will be of greatest value in giving an idea of accomplishments with the crops with which he has had little experience. The title "Recent Advances in Agricultural Plant Breeding" is somewhat misleading to the plant breeding student as technical methods of breeding are given only briefly and in some cases recent advances in methods are not adequately treated.

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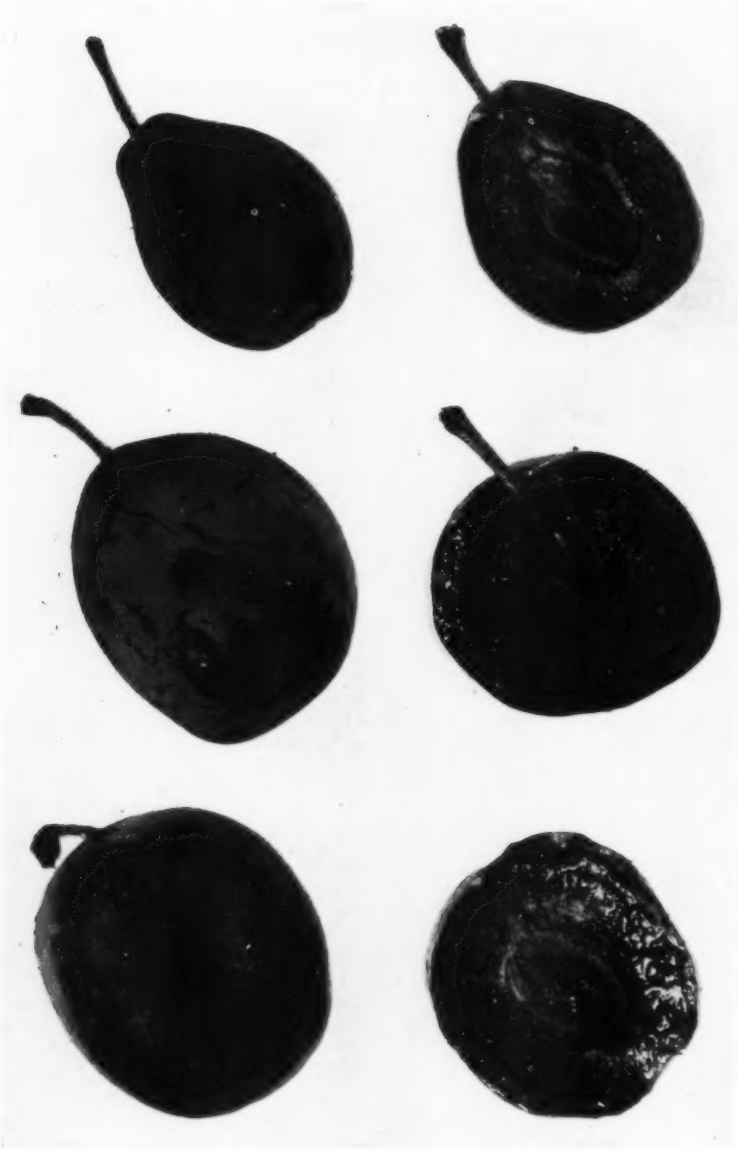

BUD VARIATIONS OF THE AGEN PRUNE

Figure 8

In the top row are shown a normal fruit and a cross-section of the Agen prune. Below these are comparable fruits of two bud variations of this variety; a variation which has been introduced as the Coates prune (center), and a variation observed at Banning, California, in 1921. Bud variations in fruit shape or color are strikingly evident and are more likely to be observed than variations in some other characteristics. Controlled experiments have demonstrated also the existence in the Agen variety of variations in productivity and in time of ripening. Such known instances emphasize the need for careful selection of budwood from trees and limbs known to have normal or superior characteristics.

BUD VARIATION IN THE AGEN PRUNE

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THE studies of bud variation in the Agen prune variety which are presented in this paper have been carried on in California orchards, mainly at Banning, where individual-tree performance-records were secured during a period of ten years in an orchard, and at Shafter where progeny trees of several bud variations which were found in the trees of the Banning orchard are being grown.

The Agen is commonly known among the growers of California as the French prune and botanically it is classified as *Prunus domestica*. It was introduced into the United States from the district of Agen, France, where it is widely grown. The first recorded importation of trees or propagating material of this variety was made in 1854 by the United States Patent Office. The most important introduction, however, and the first one to reach California, was made in 1856 when Louis Pellier, a nurseryman of San Jose, California, introduced scions from Ville Neuve, Agen, France, and propagated them in his nursery for a commercial planting in the San Jose district.

The commercial development of this prune in California began about 1880. At the present time there are about 180,000 acres of prune trees in California of bearing age and the annual production averages about 200,000 tons of dried prunes. Of the total prune acreage and production the Agen variety constitutes about 75 per cent. Other varieties grown to a limited extent in California include the Imperial, Sargeant and Sugar prunes.

Except for its tendency to produce only medium to small-sized fruits, the Agen is unsurpassed as a dried prune. However, the commercial price differentials during recent years in favor of large-sized prunes have encouraged the planting of varieties that tend to bear larger fruits. The most widely planted of these is one that has been developed from a large-fruited limb variation of an Agen tree, as is described in the following paragraphs.

A Striking Bud Variation

In 1904, in an Agen orchard located near Saratoga, California, a tree was found by the owner in which one of the large secondary branches bore prunes that were considerably larger and more oval shaped than the normal ones. Typical examples of the large sized and comparable normal prunes are shown in Figure 8 (center and top). The attention of a commercial nurseryman was called to this tree and its abnormal limb and he obtained several scions from the limb variation which he grafted into an Agen tree in his orchard located near Morgan Hill, California. The fruits that were subsequently produced by the top-worked tree were found to be identical in all observable characteristics with those that were borne by the original limb variation.

Another progeny test of this bud variation was made by the nurseryman in 1914, by top-working the trees in alternate rows of a five-year-old peach orchard located near Morgan Hill with buds from the large-fruited strain. The peach trees in the adjoining rows were top-worked with buds from normal Agen trees and from other selected bud variations. The top-working in this case was done by budding into three or four of the main limbs of each peach tree instead of by grafting as was done with the first progeny propagation of this large-fruited variation.

The senior writer visited the top-worked orchard in 1919, five years after the rebudding had been done. The inserted buds were found to have made an excellent growth and the trees were loaded with heavy crops of fruit. The

fine growth and splendid physical condition of the trees indicated that budding was equally as effective as grafting in top-working prune trees. Subsequent visits to the orchard by the writers have confirmed the observations made in 1919.

The transmission of the characteristics of the bud variation through bud propagation was proved by the performance of the progeny trees in this top-worked orchard. This result led to the commercial progagation of the strain which was called Coates in honor of the original propagator, Leonard Coates.

The commercial orchard experience with this strain of the Agen prune during recent years has demonstrated that it is apparently superior to the parent variety so far as size of fruits is concerned, when grown in certain districts while in other regions it is considered by growers to be no better than the ordinary Agen prune. In those sections where it succeeds best the Coates prunes average in size about 25-30 to the pound as compared with an average size of about 50-60 to the pound for normal Agen prunes. The production of the trees in the districts apparently adapted for this strain is reported to be fully equal to that of normal Agen trees. The commercial processing, texture and flavor characteristics are said to be equally good, if not somewhat superior to those of normal Agen fruits. A more complete description of this bud variation has been presented in this JOURNAL (Vol. X, No. 8, Nov., 1919).

Other Limb Variations

A study of individual tree production was begun in 1921 in an orchard of Agen prunes belonging to French Gilman at Banning, Calif.

Brief descriptions of two striking limb variations which were discovered in 1921 in the Gilman plot, together with a report upon the performance of the progeny trees that were propagated from them are presented in the following paragraphs. In one of the otherwise apparently normal trees in the Gilman plot a scaffold limb was found that was wholly unproductive while the other limbs on the same tree bore normal quantities of typical Agen prunes. Subsequent observations proved that this limb was unproductive every year, bearing at most not more than a dozen fruits during any one year and during most seasons it was entirely barren. The few fruits that were occasionally produced by the unproductive limb were normal in all observable characteristics.

The foliage of the unproductive limb was apparently identical with that of the normal limbs on the same tree or that produced by other trees in the plot. The only characteristic that distinguished the unproductive limb from the other limb, so far as observed, was that of quantity of fruit.

Propagations from the unproductive limb and also from a normal limb of the same tree were made on Myrobalan rootstocks in August, 1926. Four progeny trees, two from the unproductive limb and two from the normal limb were planted in the experimental orchard located at the U. S. Cotton Field Station, Shafter, California, in February, 1927. In 1929 a few fruits were produced by the progeny trees from the normal parent limb but none was produced by those from the unproductive parent limb. The performance records of the four trees for the seasons of 1930 and 1931 are shown in Table I.

The performance records indicate that the quantity characteristics of each of the two limbs that were propagated from the same parent tree have been transmitted through bud propagation. The 18 fruits borne by Tree No. 44-6 and those produced by trees Nos. 44-8 and 44-9 were typical Agen fruits.

A Late-Ripening Variation

In another tree in the Gilman plot a limb was found in 1921 that produced fruits which ripened about two weeks later than the normal ones borne by the other branches of this tree or by other

trees in the same orchard. This late-maturing characteristic of the fruits produced by the limb variation has been found to be a consistent one in the parent limb every year since it was first observed. The late-maturing fruits apparently have the same characteristics as those of the normal ones except that of the time of ripening.

Buds from the late-maturing limb variation and from a normal limb of the same parent tree were inserted in Myrobalan rootstocks in August, 1926. Five of the resulting progeny trees, two from the late-maturing variation and three from the normal limb, were planted as dormant buds at the Shafter station in February, 1927. A few fruits borne by these trees in 1929 proved that those of the progeny trees from the late-maturing limb variation ripened about two weeks later than those that were produced by the comparative normal progeny trees. The performance records of the progeny trees for the season of 1930 and 1931 are shown in Table II.

The performance records and observations made during 1930 and 1931 indicate that the late-maturing characteristic of the parent limb variation has been perpetuated through bud variation.

Additional bud variations have been observed in trees in the Gilman orchard from time to time such as that shown in Figure 8, but of necessity progeny propagations from them have been deferred.

Significance of Bud Variations

The bud variations of the Agen prune described in this paper are mainly concerned with the size and shape of fruits, the quantity of production and time of maturity of the fruits. The senior writer has observed during the past twenty years several additional variations in Agen prune trees located in orchards in the Santa Clara Valley and other prune growing districts of California. Lack of time and opportunity for obtaining systematic individual-tree performance record studies as a basis for the selection of outstanding variations for progeny testing has prevented their further study.

The bud variations that have been discussed in this paper indicate something of the nature of bud variability in the trees of the Agen prune. The progeny tests of these variations prove that it is possible to isolate and propagate strains of this variety in which the trees tend to produce fruits having larger size, different shape or later maturity than the characteristics of the normal fruits of this variety. From field observations by the writers it seems probable that in addition to those mentioned, variations may be found with characteristically early-maturing fruits, others with very small seeds in the fruits and others with a larger size of fruit than that of the Coates strain. In other words these studies suggest that superior strains of the Agen variety may be isolated and propagated from bud variations which are superior to the normal in the quantity and the commercial quality of their crops.

While Agen prune trees in observed orchards ordinarily tend to bear heavily, occasional trees have been found in most of the orchards that produce but few fruits and some have been found that were almost entirely barren. This condition has usually been explained by the growers as the result of some adverse cultural or environmental influence. However, the unproductive limb variation and its progeny described in this paper indicate that in some instances at least the unproductive trees observed in the commercial orchards may have resulted from the unintentional propagation of unproductive limbs in the trees from which buds were obtained for commercial nursery propagation.

Improvement of the Agen Variety Through Bud Selection

As a result of the studies of bud variations in the Agen prune and of similar work in peaches and pears, it seems probable that superior strains of

the Agen prune can be obtained through the isolation and propagation of strains originating from commercially valuable bud variations. Furthermore, in the propagation of nursery trees of the Agen variety it is suggested that systematic care be taken to obtain budwood only from those trees that are free from striking variability of fruits or foliage and that the parent trees selected as sources of budwood for commercial use should be known to bear consistently heavy crops of uniformly desirable fruits.

Summary

Bud variation in trees of the Agen prune variety has been proved through progeny tests of several striking limb variations which have been found in otherwise normal Agen trees.

The variations described in this paper are of scientific interest and some of them are of obvious commercial importance. The progeny tests indicate that strikingly different strains of the Agen prune may be isolated and propagated from bud variations.

The unproductive variation and its progeny indicate that some of the low yielding trees in established orchards are probably the result of the unintentional propagation of unproductive limb variations and that in order to avoid the commercial propagation of these or other undesirable variations it is important to obtain budwood from parent trees where careful individual-tree performance-record studies have been carried on over a period of years.

TABLE I.—Production records for 1930 and 1931 of four progeny Agen prune trees planted as dormant buds, Feb. 1927, at the U. S. Cotton Field Station, Shafter, Calif. Two of these trees were from an unproductive limb and two from a normal limb of the same parent tree.

Progeny tree	Source of budwood	Number of prunes 1930	1931	Total
44-6	Unproductive limb, Gilman SW-8-38	1	17	18
44-7	Unproductive limb, Gilman SW-8-38	0	0	0
44-8	Normal limb in same tree	233	440	673
44-9	Normal limb in same tree	84	180	264

TABLE II.—Individual tree performance records for 1930 and 1931 of five Agen prune trees, three of which were propagated from a limb variation of an otherwise normal tree that consistently produced late maturing fruits and two from a normal limb of the same parent tree. The progeny trees were planted at Shafter, California, in February, 1927.

Progeny tree	Source of budwood	Number of fruits 1930	1931	Maturity dates 1930	1931
43-1	Late-maturing limbs, Gilman-NE-4-22	11	2	Sept. 18	Sept. 20
43-2	Late-maturing limbs, Gilman-NE-4-22	24	2	Sept. 18	Sept. 20
43-4	Normal limb	6	3	Aug. 30	Sept. 1
43-4	Normal limb	3	75	Aug. 30	Sept. 1
43-5	Normal limb	0	30		Sept. 1

LINKAGE TESTS IN POULTRY

II.—Linkage of the Rumpless Gene

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INCIDENTAL to our work with rumpless chickens, matings were arranged during the past two seasons testing the existence of linkage between the genes for rumplessness and crest and the genes for rumplessness and frizzled plumage. Since these matings cannot be continued, it seemed desirable to put the available data on record, although for final conclusions they are admittedly inadequate.

All crosses to be discussed here involved our stock of intermediate rumpless fowl. All intermediate rumpless birds carry the gene for complete rumplessness. The males in the matings used for linkage tests were known to be heterozygous for the rumpless gene. The genetic nature of the modifiers which bring about the intermediate rumpless condition has not been fully analyzed as yet, and for the purposes of the present report both rumpless and intermediate rumpless offspring of these matings have been grouped together since all are known to have received the rumpless gene from their father. In other words, the linkage relations of the rumpless gene only are discussed here, while those of the modifiers are disregarded.

Rumplessness and Crest

Usually it is possible to decide at hatching time whether or not a chick will develop a crest later on. Since it was found, however, that the chick descriptions were not invariably reliable, our analysis of the linkage relations were based on those birds only which lived to maturity. In 1931 we had three matings of cockerels heterozygous both for the genes for rumplessness and for crest to normal White Leghorn pullets. In 1932 we had two such matings. The segregation among the offspring of these matings is shown in Table 1. In all five matings, the total offspring, as far as we have complete descriptions, numbered 502. The segregation was as follows: 123 rumpless and crest, 112 rumpless and no crest, 144 normal rump and crest, and 123 normal rump and no crest. The expectation was 125.5 in each class. There were 246 birds with the parental phenotype and 256 recombinations (expectation 251 for each). This obviously means that the genes for rumplessness and for crest are either inherited independently or that linkage is so loose that very large numbers would be required to establish such a relationship. Warren's data[3] from similar matings closely agree with our own.

Rumplessness and Frizzled Plumage

The heterozygous frizzle condition cannot be determined at hatching time. We have included in the present analysis only chicks which were at least two months old. In 1931 as well as in 1932 we had matings of two cockerels each, heterozygous for the genes for rumplessness and frizzled plumage to normal White Leghorn pullets. The total classified offspring of these four matings amounted to 449 individuals. The detailed data are presented in Table II. Segregation was as follows: 117 rumpless and frizzle, 89 rumpless and normal plumage, 101 normal rump and frizzle, and 142 normal rump and normal plumage, with an expectation of 112.25 for each class. There were 259 parental phenotypes and 190 recombinations as against an expecta-

tion of 224.5 each. This is a serious deviation from random sampling. The number of recombinations amounts to 42.3% of the total with a standard error of 2.2 (determined from Charles' graph for computing standard error[1]). We cannot but conclude from the results of these matings that the genes for rumplessness and for frizzled plumage are located in the same chromosome at relatively distant loci. The fact that the two matings with the largest number of offspring (pen 33, 1931 and pen 34, 1932) gave almost identical results, seems to strengthen this conclusion. It is very desirable, of course, that further tests are made.

Suttle and Sipe[2] reported linkage between the genes for frizzled plumage and crest. In crosses of normal males and heterozygous crested frizzle females, they found among a total of 235 offspring, 167 non-crossovers and only 68 crossovers. The number of crossovers equals only 28.9% of the total with a standard error of 2.94. This would seem to be good evidence in favor of the authors' conclusion that the two genes are linked. Since our own data point to the existence of linkage between the rumpless and frizzle genes, linkage should also be expected to exist between the rumpless and crest genes. This conclusion, however, is not substantiated by our own data nor by those of Warren. If further tests verify the existence of linkage between the frizzle and crest genes as well as between the rumpless and frizzle genes, it must be concluded that the genes for crest and rumplessness are located on opposite sides of the frizzle gene, and that the linkage between rumpless and crest is very loose.

Conclusions

Our data suggest the existence of linkage between the genes of rumplessness and frizzled plumage.

No evidence has been found for linkage of the genes for rumplessness and crest.

Literature Cited

1. Charles, D. R. A graph for standard errors of ratios. *Genetics* vol. 17, 1932.

2. Suttle, A. D. and G. R. Sipe. Linkage of genes for crest and frizzle. *Journal of Heredity* vol. 23, 1932.

3. Warren, D. C. Autosomal characters independently inherited in the domestic fowl. *Proceedings* of the Sixth International Congress of Genetics, Ithaca, New York, 1932, vol. 2, 1932.

TABLE I. Segregation in crosses of White Leghorn pullets and heterozygous crested rumpless cockerels.

Year	Pen	Rumpless crest	Rumpless no crest	Normal rump crest	Normal rump no crest
1931	3	10	14	19	13
1931	7	9	11	5	8
1931	14	32	28	43	41
1932	4	41	31	38	34
1932	29	31	28	39	27
Total		123	112	144	123

TABLE II. Segregation in crosses of White Leghorn pullets and heterozygous frizzled rumpless cockerels.

Year	Pen	Rumpless frizzle	Rumpless normal plumage	Normal rump frizzle	Normal rump normal plumage
1931	26	9	8	16	15
1931	33	42	26	31	55
1932	28	18	28	32	30
1932	34	48	27	22	42
Total		117	89	101	142

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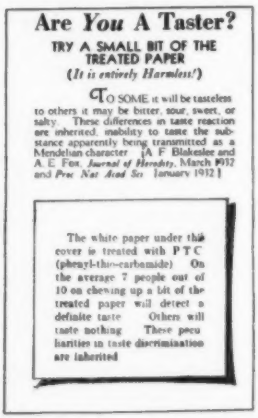
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